Note on Professor Newcomb's Note in the "Monthly Notices" for 1894 *November.* By E. J. Stone, M.A., F.R.S., Radcliffe Observer.

It is evident that Professor Newcomb is either unable or unwilling to seriously discuss the fundamental question which I have raised with respect to the accuracy of our determinations from observation of the variable, t, in terms of which our theoretical results are, or can be made, exact.

And it is useless to attempt to continue a discussion without coming to close quarters on the point at issue.

But it is not desirable that my views should be misrepresented; and it is a misrepresentation of my views for Professor Newcomb to assert that "Mr. Stone maintains that this result" (i.e. the numerical difference obtained by Professor Newcomb) "is due to my failure to apply a certain correction to the numbers of Le Verrier's Tables, which correction is not a function of anything contained in the tables, or of the epoch of any natural event, but of the date when a certain office, three thousand miles away, began to use the tables in computing the solar ephemeris." Such a statement has certainly never been made by me.

The expression for the geocentric longitude of the Sun as a function of 't,' is one thing; the expression for the right ascension of a meridian as a function of 't,' is another thing.

What I have maintained, maintain, and assert that I have proved, is as follows:—That if astronomers use for years, in the determinations of the local sidereal times, a "unit day" which is the $\frac{N}{2\pi}$th part of a mean tropical year, and turn their theoretical expressions into "time" at the rate of 2π to 86,400 mean sidereal seconds; but, for any reasons whatever, at some definite epoch introduce a change and use a "unit day" which is the $\frac{N'}{2\pi}$th part of a mean tropical year: then, simultaneously with the change of units, the mean angular motion, Ω, of the meridians in right ascension must be changed to Ω', so that

$$\Omega : \Omega' = N : N';$$

but in this case we have

$$\Omega - \Omega' = (N - N')\left(1 + \frac{2\pi}{N_0}\right) = N - N' + \frac{2\pi}{N_0}(N - N'),$$

where N_0 will be a definite angle, which, in practice, will not differ much from N or N', but cannot be equal to both N and N'. This result is generally true, and, therefore, true when the N is that of Carlini's, and the N' that of Le Verrier's, Solar Tables.

Now, although Professor Newcomb asserts that the corrections which I introduce are not functions of any natural event, it is a

simple matter of fact that he recognises that changes in Ω and Ω' result from changes in N and N', and he carefully computes the effects of these changes in $\Omega - \Omega'$, and allows for their effects in his work so far as the term $N - N'$ is concerned, but he overlooks and, therefore, neglects the far more important effects which result from the existence of the term

$$\frac{2\pi}{N_0}(N - N').$$

The differences between the numerical results which Professor Newcomb obtains for the local sidereal times at the commencement of each "unit day" and those which I obtain are simply due to the fact that I allow for the effects of the term

$$\frac{2\pi}{N_0}(N - N')$$

and Professor Newcomb neglects them.

If, therefore, Professor Newcomb can prove that no such term as

$$\frac{2\pi}{N_0}(N - N')$$

exists in the difference $\Omega - \Omega'$ under the conditions stated, then his results are right and mine are wrong; but, on the other hand, if the existence of this term is necessary to preserve continuity in our measures of time, then Professor Newcomb's results are wrong and mine are right.

The real question at issue is whether the tabular geocentric places, found as at present, and expressed as a function of t, can be correctly referred to the meridian of an observer without taking account of both the terms

$$N - N' + \frac{2\pi}{N_0}(N - N')$$

in the expression for $\Omega - \Omega'$.

Professor Newcomb apparently considers that it is only necessary, when different fractional parts of a mean tropical year are adopted as the "unit day," to take account of the term $N - N'$; and the result is that Hansen's Lunar Tables, which, when thus compared with observation, gave, comparatively speaking, small residuals from 1750–1863, ceased to represent the observations in 1864, when the data of Carlini's Solar Tables were generally replaced by those of Le Verrier, to such an extent that, although the mean annual residual in 1863 was $-1''\cdot54$, there has been no negative mean annual residual since that year, and the residual for 1892 amounted to $+19''\cdot69$, and is now over $+20''$. But when allowance is made, as I maintain must be the case to preserve the continuity of our measures of time, for the term

$$\frac{2\pi}{N_0}(N - N')$$

the same tables represent the observations before and after 1864 with equal accuracy.

These are the facts with which observation furnishes us. I leave my readers to draw their own inferences.

See *Monthly Notices* 1894 December.

The Transit of Mercury, 1804 *November* 10, *observed in New South Wales.* By H. C. Russell, B.A., F.R.S.

The morning was fine and clear, except a slight haziness. Unfortunately, the Sun was so situated that, viewed from the large equatorial, it was behind the Time-ball Tower. I was therefore obliged to use a very fine 6-inch equatorial by Sir Howard Grubb, which occupies the north dome. This telescope defines admirably, and was for the occasion reduced by a diaphragm to 3 inches. A reflecting prism was used to reduce the light still more, and a power of 120. When the Sun was clear of lower mists, I began to observe the planet carefully. It was remarkably black compared with the Sun-spots, quite round, and fairly well defined. During the whole time I saw no white spot on the planet, or anything to mar its blackness, and there was no surrounding ring or fringe of light. Examined closely, it was seen that at short intervals definition became bad, the apparent vibration producing an ill-defined edge. The rays of the Sun were powerful, and its heating effect no doubt gave rise to currents of unequal temperature, and as these passed before the telescope the planet's limb seemed to be rippling, as if waves were passing over it. As the planet approached the Sun's limb the narrow space between seemed to throw these conditions into prominence, and it became evident, when the planet got within one-third of its diameter (estimated) of the Sun's limb, that the contacts were not to be observed under favourable conditions. A moment of bad definition came on, in which the ripples seemed to form dents in the limbs of planet and Sun from $1''$ to $2''$ wide, and suddenly a shady ligament joined the limbs of planet and Sun. It was darker near the planet than the Sun's limb, and seemed to be made up of soft shaded lines from limb to limb, making up a band about one-quarter of the planet's diameter wide. This lasted two or perhaps three seconds, and then disappeared, the definition having suddenly improved. The time at which the ligament formed was November 10, $19^h\ 16^m\ 1^s{\cdot}5$. The greater darkness of the ligament near *Mercury* would result from the planet being darker than the sky, and its vibrations would obviously make a darker shading than that caused by the overlapping in vibrations of the darkness outside the Sun's limb. The space between the planet and Sun's limb now became quite clear and free from dark shading, until $19^h\ 16^m\ 18^s{\cdot}5$, when